ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *December* 9, 1853. No. 2.

G. B. AIRY, Esq., in the Chair.

Lieut. E. D. Ashe, R.N.,

was balloted for and duly elected a Fellow of the Society.

Discovery of a New Comet. By M. Klinkerfues.

On the morning of Dec. 2, M. Klinkerfues discovered a new comet in the constellation of *Perseus*. The time was too short to determine with certainty its position with respect to the neighbouring stars. The following is given by him as its approximate place:—

1853.	Göttingen M.T. h m s	R.A. h m s	Decl. ° ′ ″
Dec. 2	16 3 49·5	1 37 19·5	+51 36 37·6

M. Klinkerfues estimates the daily motion in right ascension at — 30′ of space, and the daily motion in declination at — 90′.

The following elements of this comet have been calculated by M. Klinkerfues:*—

T	1854, Jan. 9, 8251	
	° ′ ″	
π	.....	53 22 54·6
λ	.....	227 23 1·8
i	.....	66 46 24·0
Log *q*	...	0·306940

Retrograde.

Observations of Comet III., 1853. By W. Bradshaw, Esq., Master and Commander of H.M. Troop-ship, Resistance.

(*Extract of a Letter to Rear-Admiral Sir Francis Beaufort, dated Bathurst, Gambia, Oct.* 8, 1853.)

"Having during the passage of the Resistance from Port Royal to the Gambia observed a comet, no account of which was given in any of the newspapers seen by me at Jamaica, I beg leave to forward for your information the following observations made on board:—

"August 22d. In lat. 24° 50′ N., long. 73° 25′ W., apparent time at ship 7·25 p.m., observed a comet bearing W.N.W., the tail

* Extracted from the circular of Professor Chevallier.

extending E.S.E. 5° or 6°. Observed distance between the comet and *Benetnasch*, 34° 31′ 30″. Comet south of the star. Observed distance between the comet and *Arcturus*, 36° 30′ 0″. Comet north of the star.

" August 24th. In lat. 28° 29′ N., long. 71° 39′ W., apparent time at ship, $7^h\ 17^m\ 23^s$ p.m. Distance between the comet and *Benetnasch*, 37° 6′ 0″. Distance between the comet and *Arcturus*, 35° 52′ 30″. Altitude of comet, 11° 43′ 40″, computed R.A., 10° 43′ 41″, declination, 32° 10′ 10″ N.

" August 25th. In lat. 29° 55′ N., long. 70° 30′ 0″ W., apparent time at ship, $7^h\ 26^m\ 56^s$ p.m. Distance between the comet and *Benetnasch*, 36° 50′ 55″. Distance between the comet and *Arcturus*, 35° 42′ 10″. Altitude of comet, 9° 12′ 0″.

" August 26th. In lat. 31° 12′ 0″ N., long. 69° 36′ 0″ W., apparent time at ship, $7^h\ 25^m\ 15^s$. Distance between comet and *Benetnasch*, 38° 3′ 0″. Distance between comet and *Arcturus*, 35° 42′ 20″. Altitude of comet, 7° 46′ 4″.

" August 27th. In lat. 31° 48′ N., long. 68° 59′ 0″ W., apparent time at ship, $7^h\ 29^m\ 0^s$. Distance between comet and *Benetnasch*, 39° 25′ 0″. Distance between comet and *Arcturus*, 35° 52′ 50″. Altitude of comet, 7° 47′ 20″.

" The comet apparently travelling rapidly towards the sun. The nucleus much brighter, and the tail extending to the E.S.E., about 10° in length.

" The observation of the 27th was the last one taken on board, the comet not having been seen after that date.

" Since my arrival at the Gambia I have read some accounts of this comet; but the favourable circumstances under which my observations were made induce me to persist in my intention of submitting them to your notice."

Second Elements of Euterpe.

Computed from the observations made at Regent's Park, Nov. 8, at Durham and Hamburg, Nov. 28, and at Hamburg, Dec. 11, 1853, by Professor Chevallier and Mr. George Rümker.

Epoch Jan. 0·0, 1854, Greenwich Mean Time.

	°	′	″	
M	348	53	31·5	
π	84	27	30·4	Mean Equinox at Epoch.
λ	93	30	14·5	
i	1	36	24·0	
φ	9	21	24·7	
Log a ...	0·369656			
Log μ ...	2·995523			

These elements represent the middle observation, with an error (computed — observed) of −2″·7 in longitude, and 0″·0 in latitude.

They have been computed from the last available observations; the unfavourable state of the weather having prevented any observations of *Euterpe* from being made at Durham since December 3.

Professor Chevallier has forwarded to the Society several copies of a printed circular containing the above elements, accompanied by an ephemeris of the planet computed from them, and extending from December 27 to February 1, 1854. The circular also includes an ephemeris of Comet V. 1853, extending from December 25 to January 14, 1854, based on an orbit computed by the discoverer, M. Klinkerfues.

A copy of this circular may be obtained by any Fellow of the Society upon applying at the Apartments, Somerset House.

Observations of Euterpe. By R. C. Carrington, Esq.

Redhill. Transit Circle.

1853.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Log $\frac{q}{P}$
Nov. 18	11 18 22·1	3 9 18·11	74 26 34·0	9·7630
19	11 13 26·7	8 18·47	29 27·9	9·7635
21	11 3 38·5	6 21·72	35 6·7	9·7645
22	10 58 45·3	5 24·26	37 51·1	9·7650
23	10 53 53·0	3 4 27·79	40 30·5	9·7654
Dec. 1	10 15 38·6	2 57 39·56	74 58 21·9	9·7685
16	9 9 3·4	2 50 1·80	75 8 42·8	9·7704

Note on Comet III. 1853. By M. C. Rümker.

The phenomenon attending the setting of the comet of Klinkerfues, witnessed September 2, 1853, at Liverpool, Durham, Markree, in America, throughout Denmark, Sweden, and several parts of Germany, brings to recollection the appearance of the great comet of 1843. The tail of that comet first attracted the attention of European observers, some of whom never saw the nucleus; but the tail remained for several evenings visible; and on or about the 25th of March, after its setting in the south-west, a luminous appearance was observed above the western horizon and called a zodiacal light, the name of *Aurora occidentalis* being deemed objectionable.

On the 2d of September, 1853, at $9^h\ 15^m$, or two hours after the setting of the comet of Klinkerfues, I observed at Hamburg, a little to the north of west, an appearance resembling that of the tail of a comet, slightly curved, with the concave side to the north.* But as the phenomenon, which appeared at first immovable, was a quarter of an hour afterwards seen to the south of *Arcturus*, when clouds prevented further observations, and as several observers have attributed it to the effects of the comet, I propose to in-

* Dr. Ritter observed precisely the same phenomenon at Göttingen, where M. Klinkerfues had seen some days before the actual tail of the comet, curved in the same direction.

vestigate how far this supposition can be justified by theory and reconciled with the coincidence of the alleged tail with the star *Arcturus*.

If the tail of the comet had no curvature, it should, as viewed from the earth, coincide with a great circle passing through the sun and comet; but the angle formed by this great circle with an arc joining the comet with *Arcturus* would be the geocentric effect of a curvature. Now it may be remarked, that the projection of this great circle upon the plane of the meridian must be represented by a straight line when its intersection with the equator crosses the horizon, but will, in the present case, where the right ascension of the sun is less, but its declination greater, than that of the comet, be curved previously to that time with its convex side to the north, and subsequently to the north pole. The calculation of the effect of curvature may then be based upon the geocentric place of the comet, derived from observation, by computing the angle at the comet in a triangle, in which the sun's and comet's polar distance and the intercepted angle, or the difference of their right ascensions, are given; and by subtracting from its supplement that angle at the comet contained between the arcs joining the comet with the pole and with *Arcturus*, the remainder is the geocentric effect of curvature, and will be found $28^\circ\ 12'$, assuming the right ascension of the comet to have been $11^h\ 22^m\ 29^s$, and its declination $= +6^\circ\ 54'$. It is found that the angular distance of the comet from the sun is $8^\circ\ 51'$, and from the star $42^\circ\ 23'$.

Although instances of greater curvatures are known, it may be worth while to ascertain the amount of the actual curvature, or the angle at the sun between two lines from the sun to the comet, and from the sun to that point in the comet's tail which, viewed from the earth, covered *Arcturus*. But, as physical causes can only be assigned for curvatures taking place in the plane of the comet's orbit, we shall imagine two great circles meeting in the geocentric place of the sun, and in the heliocentric place of the earth, the planes of which intersect each other in the produced radius vector of the earth. In the circumference of the *first* of these two great circles, the plane of which passes through the real comet, are its heliocentric and geocentric places, at the point where two lines from the sun and from the earth through the comet terminate. This circle coincides with the above, passing through the sun and comet, made use of in our former calculation. In the circumference of the plane of the *second* great circle passing through *Arcturus*, this star is at the same time the geocentric place of that point in the axis of the comet's tail, which appeared to us to cover it, whilst the intersection of this second great circle with the comet's orbit is the heliocentric place of this same point, and the arc of the comet's orbit intercepted between the first and second great circle is the actual curvature of the tail.

For the calculation we have, in addition to the above-stated observed place of the comet, the choice of more arguments than are required, which may serve to check the calculations. For my calcu-

lation only two parts of the elements are introduced, viz. the comet's radius vector, which, so near the perihelion as well as the inclination, may be sufficiently relied on. I omit the calculation, which may be arranged differently. The geocentric effect of the curvature as well as the comet's angular distance from the sun and from *Arcturus*, resulting from this calculation, agree perfectly with what was found by the former proceeding. The actual curvature, or the angle at the sun between the comet and the point in question, I find = 22° 5′, and the absolute distance of this point from the earth I find 0·64947, which is all within the limits of probability, so that we need not hesitate to ascribe the beam of light seen first in the west and then in the south-west to the comet. But, in order to establish a connexion between the first apparition and ensuing phenomenon (zodiacal light or aurora borealis), we must follow the comet in the different positions of the first great circle with respect to the horizon of Hamburg, during the rotation of the earth; whence it becomes evident, that the point of its intersection with the equator sets in the true west, and that the amplitude of that point of the beam of light which crossed the horizon at $9^h\frac{1}{4}$ was 2° 58′ north of west. As long as the sun was between the comet and the lowest point of the great circle, elongated 90° from its intersection with the horizon, the beam was directed upwards; but after the comet's passing this point, the beam was directed downwards, and the phenomenon must disappear. It further follows that, under the above-mentioned circumstance of the sun's north polar distance being the lesser, the amplitude of the setting point of the beam must, until its disappearance below the horizon, have had a southerly motion, whilst the comet itself below the horizon was proceeding to the north. All this agrees perfectly with the observations in Hamburg and all other places, where, in general, the first phenomenon was seen more towards the south, where it was perceived later, setting aside anomalies arising from difference of latitude. At $9^h\ 15^m$ the

Sun's depression below the horizon of Hamburg was	19	°	′ 34;	azimuth	N.	43	°	′ 46 W.
Comet's — — — — —	16	25	—		N.	52	25	W.

At 11 o'clock the depression of the comet was 26° 17′, and its azimuth N. 26° 38′ W., being then 1° above the sun. In more northerly latitudes the comet approached nearer the horizon, so that the apparition of its more expanded fan-like tail could, with the aid of increased darkness, produce a phenomenon similar to an aurora borealis. I beg leave to mention that Mr. Hartnup, as I understand, has called it an aurora borealis of unusual appearance. Towards two o'clock in the morning of September 29 an aurora borealis was seen at Durham. Bruhn's comet was then quite near the horizon, 36° perpendicular above the sun. I have further to impress that the comet of Klinkerfues was *in its perihelion*, and the comet of Bruhn's and the great comet of 1843 near it.

There is, however, one circumstance which is more difficult to

reconcile with this hypothesis, that is its proper motion, differing from that of the fixed stars, and greater than could arise from its motion in its orbit. But, considering that the tail of a comet is a fluid which cannot be classed under what is generally termed heavenly bodies, its motion cannot be put to the mathematical test before its nature is better known. It is, moreover, well known that motion has been observed in the tails of several comets, and that it consists of rays which project in short intervals of time, considerably more or less, like those of an aurora borealis.

As these points appertain to physical astronomy, I must leave them to those who are better versed in that part of the science, and confine myself to the following remarks. The distance of the comet from the earth was too great to allow a contact of its expanded tail with the atmosphere of the earth. But, notwithstanding, the phenomenon might have been an atmospherical one caused by the tail of the comet, as there are perihelia round the sun and halos round the moon, rainbows, &c. &c., which expand and contract, and are generated in the atmosphere. And although there have been observed more auroræ boreales than comets, there certainly also exist more comets than those that have been discovered; and it has already been remarked by others, that our earth may often have been enveloped in the tail of a comet without our having been aware of it.

Extracts of a Letter from Professor C. Piazzi Smyth relative to Cometary Physics and to the Rating of Chronometers by Lunars.

"It is generally believed that the tails of comets are only put on near the perihelion point of their orbits; that they are produced by the intensity of the sun's rays there, and are dissipated on either side. Now I maintain that this is the reverse of what actually takes place. The comets have all the particles of their mass, whether tail, body, coma, &c., always with them; though in very different states of concentration at different times: and this it is which, combined with the greater or less intensity of solar illumination, according to their distance from the sun, and the greater or less angle under which they may be seen by reason of proximity to the earth, causes the variations in observed appearance. Moreover, it is now found that all comets, large and small, invariably become not only smaller, but more compressed and concentrated, as they approach the sun; and so far from a dissipation of solid matter, there is a bringing together of it, and a consequent brightening. The tail, indeed, shuts up like a many-draw telescope as it nears the perihelion, and opens out again as it leaves it. The amount of such concentration appears to vary in a proportion not yet accurately ascertained with the length of the radius vector or distance of the comet from the sun. So that, with such comets as Encke's and Biela's, where the difference of perihelion and aphelion distances is not very great, the amount of concentration is never considerable, and they are never

seen but as faint, vaporous, telescopic bodies. But when the aphelion distance is very great, and the perihelion distance very small, as with the comet of 1843, and to a much less extent with the third one of 1853, then so great a concentration occurs, there is such a bringing together of finely divided matter, that, though otherwise invisible, it is now able to reflect the sun's rays so strongly as to become abundantly apparent to the naked eye in the evening, and finally, at the perihelion passage itself, for a short time even in open daylight."

"I may further remark that, in consequence of the concentration of matter which ensues as the comet approaches the perihelion, much confusion arises from speaking of the length of a comet's tail without a qualifying note of the part of the comet's orbit at the time. The same reasoning applies to every other part of the comet, for it is all vaporous, but the nucleoid centre. Here, if anywhere, is the solid body, but it is infinitely small. The old writers do, indeed, talk of very large nuclei in former comets, and that of the present one has been described to have been, on August 27, upwards of 9000 miles in diameter, or enough to revive the old fears lest it should come in contact with the earth and break it to pieces. But that was not the nucleus, it was only the head round about it, that head being composed of the thinnest and lightest of conceivable vapour, merely rendered apparent there by the particles being brought very close together, and the whole so near the sun as to be lighted up by a more than ordinary blaze of light, and then to shine forth like a luminous object, as a thin cirrus cloud so often does when seen in the neighbourhood of the solar orb."

"Exactly the same appearance was observed at the similar part of its orbit in the great comet of 1843, when everything being on so much larger a scale, its internal economy could be better studied, and a powerful reflecting telescope actually showed the transparency, exhibited stars shining freely through that which persons with smaller telescopes were calling the solid nucleus. Day by day, too, as the comet left the perihelion, the once solid-looking head expanded and enlarged, and its outline became indistinct, and the whole was at length lost to all view, from being so rarified as neither to be able to reflect back any of the rays of the sun, nor to stop that coming through it from the smallest of the stars in front of which it passed."

* * * *

"As to the difference between the results of different lunars, Captain Toynbee should be advised of the probability of its arising from errors of the Tables; *i.e.* of the moon's place in the *Nautical Almanac*.

"For this amounts, in some parts of a month, to 15″ or 20″; and as it varies with opposite signs throughout the month, that is a reason why he finds lunars a fortnight apart, taken together, give a better result than two taken on following days.

"Again, if he can show that sensible errors to sailors arise from

the errors of the Lunar Tables in places in the *Nautical Almanac*, it will be a very strong argument for new Lunar Tables being made, based on all the recomputed Greenwich Observations, including the discoveries of Hansen, Adams, and others.

"Further, he should never depend on *Nautical Almanac* or anything else for the diameter of any heavenly body, but measure it for himself with his own instruments, and, comparing the result with the *Nautical Almanac* for the time, get the correction due to that cause: and always apply this correction in any case wherein the *Nautical Almanac* value must be used, as when the moon is near conjunction."

Remarks by Mr. Carrington on presenting to the Society printed Copies of Nine Circumpolar Charts.

"I have the pleasure this evening to present to the Society printed copies of nine draft maps, on a scale of an inch and a half to the degree, containing all the stars above the 11th magnitude, and many of the 11th within 9° of the north pole. These maps are the result of a first examination of the region I have selected for observation, and were formed principally with the object of making a provisional catalogue for my own use, during the second stage of accurate observation on the meridian. They have been entirely executed with my $4\frac{1}{2}$-foot equatoreal, described in the last *Monthly Notice*.

"The process followed in their construction was first to zone over the region in two broad zones, of which the first extended from 4° to $9°\frac{1}{2}$, N.P.D., and the second from 5° above to 5° below pole. From March 25th to May 20th of the present year, 20 zones were thus observed, which, when reduced and cleared of duplicates, yielded about 2000 net points for a foundation for the maps. The degree of accuracy attained was quite sufficient for the purpose. The stage of filling in was then proceeded with, and terminated on Nov. 8th. In each case a skeleton map was first prepared of the stars observed in the zones, and then filled in by estimation, considerable pains being taken to get satisfactory alineations. The map was then copied on a glass desk, through which the light of the sun was made to pass; and the copy was again compared with the heavens, a general revision and the adjustment of the relative magnitudes being chiefly attended to. It is the date of this second comparison which is given on the maps. From this corrected copy two copies were taken; one in common ink for my final standard copy, and one in anastatic ink for transfer by the printer, whose proofs were compared with and corrected by my standard copy.

"My printer was Mr. Rudolph Appel, of No. 43 Gerrard Street, Soho, and his charge for fifty copies of the same map, including cost of transfer and paper, twelve shillings. Believing that the anastatic process of printing has not been previously applied to the ready and cheap production of star maps, I beg to call attention to these maps, as specimens of what may be done, and to add at

the same time that the excellence of the result increases with the artistic skill of the author.

"I particularly wish it to be understood that I regard the present maps as imperfect, and merely as drafts, although tolerably complete as maps go, and well filled in. Any one who has worked on a map will have found, that to produce one which shall be satisfactory as a picture to the eye of another observer using a different telescope, it is very necessary for him to go over his map at different times, using different apertures. This has not, at present, been done for these maps. The sensible presence of twilight during the months of June and July has been another source of imperfection. For, as the nine maps are very nearly of equal spherical area, they ought to be very accurate guages of the polar region; but, owing to the cause named, and to the effect of thin fog on every night given to map No. 3, this relation is nullified for the present.

"I have distinguished by wings all those stars which are stated by Argelander in his *Uran. Nova* to be visible to the naked eye; by the letter D all those stars which, under a power of 28, appeared to consist of two components, too close to be represented together on the map; and by the letter N all places which appeared nebulous, and at which I should halt in sweeping for comets, although it is very possible that some of them may be clusters easily resolvable under moderate powers. Other stars I have in all cases distinguished by dots of different sizes; a method of representing magnitudes which, as more pictorial, I prefer to any other.

"I may mention, in conclusion, that during the formation of these maps I was soon struck with the frequency with which little meteoric sparks passed my field of view, often leaving behind little trains of light, and that, finding it an almost constant phenomenon, I took to noting down their paths on my working-maps as I proceeded. In this way I have collected 49 little paths of very various directions, but among which one may, perhaps, trace the existence of a little shoal crossing the pole in lines generally parallel to the meridian from 1^h to 13^h R.A. On the second night of the Laurentius stream this year, there was a marked increase in the number seen; seven were marked down, while others flew through the field too quickly to be caught. As I also perceived increased activity on several cold and windy nights, these minute telescopic meteors appear to consist of two classes, like their larger brethren, those of the naked eye, namely, one of a cosmical and one of an electrical origin."

Redhill Observatory.

On a Mode of Mounting a Telescope equatoreally.

By Mr. Joseph Hopkins.

"Having recently taken out a patent for 'a mode of obtaining a straight line parallel to the earth's axis, or of making the axis of a tube or the axis of a telescope parallel thereto,' which (amongst

other purposes) may be applied to the construction of sun-dials by which minute portions of time might be marked, or to the production of equatoreal motion in a telescope without the preparatory aid of levels or divided circles, and without the usual preliminary requirements of a knowledge of the latitude and direction of the meridian, I am anxious to lay before you the application of the principle to an instrument which may, perhaps, be not inaptly named 'the portable observatory.'

"Upon a common tripod telescope stand, a brass tube is so mounted as to have a horizontal and a vertical motion, each of which may be clamped so that the direction of the axis of the tube being once determined its position may be rendered permanent.

"The tube is 5 inches in length. If now at one end of it were inserted an eye-piece, in the centre of which was drilled a hole about one-tenth of an inch in diameter, and at the other end an object cover with a central circular aperture of the diameter of a quarter of an inch, the eye of an observer on looking through the smaller of these apertures would take in a field of view, the circumference of which would be equal to that of the circle which *Polaris* apparently describes round the true polar point, and if by the requisite horizontal and vertical motions these circumferences be made to coincide their centres will also coincide, and the axis of the tube will be parallel to the earth's axis.

"The coincidence here spoken of may be nearly enough obtained where a plain tube only is used, by viewing the pole-star on that part of the circumference of the circular diaphragm nearest to the constellation *Cassiopeia*; but, as in the instrument now brought before your notice, a small telescope is used, the axis of which is to be made parallel to the earth's axis, I shall proceed to show the mode of obtaining this parallelism.

"On the outside of the tube of the small telescope are two circular rings, which are carefully ground and fitted into collars in the brass tube: an axis of the telescope is then made to coincide with the common axis of these collars, and the centre of its diaphragm is made coincident with this axial line, the diameter of the diaphragm being such as to give a field of view to the telescope of $2^\circ\ 57'$ being double the north polar distance of *Polaris*.

"A brass sector, shown in the drawing, is made to move round the upper part of the brass tube at right angles to it, carrying an index and two small perforations, through which latter mentioned, when the stars ε and ζ *Ursæ Majoris* are seen by the observer, the eye taking in at the same time the field of view of the small telescope, the position to which *Polaris* must be brought on the circumference is pointed out by the line of light seen through the index.

"As a test of the correctness of its place thus found, turn the small telescope about its axis in the collars of the tube, when, through an entire revolution of the telescope, the star should continue to cling, as it were, to the same point of the circumference of the diaphragm.

"The axis of the collars of the tube being thus made parallel to the earth's axis, we remove the small telescope and sector, insert the axis, which is fastened by clips to the larger telescope, and fitted by rings of bell metal into the collars of the tube, attaching to it the right ascension circle with its clamp.

"It is evident that to whatever height the telescope be elevated, if it be turned about this axis, it will move in planes parallel to the equator; and if it be directed to a star, a motion given to it from east to west by the hand or by a hook's joint, will serve to keep the object in the field of view.

"I have chosen, however, to give motion to the telescope by the action of a floating syphon, the weight carried by the float being connected with the right ascension circle by a cord clamped in one of its grooves.

"The upper edge of this circle is exactly 24 inches in circumference, the lower edge 25 inches nearly, the former having a proportion to the latter equal to that of the sidereal to the mean lunar day. Each of these circumferences is grooved to the same depth. The thickness of the cord should be double the depth of the groove.

"The large cylindrical pan is about $18\frac{3}{4}$ inches in diameter: it contains in a depth of 12 inches 95·7 pints. This quantity of water should be run off by the syphon in 12 sidereal hours; the rate (which should be adjusted while the telescope is in motion) is obtained by raising or lowering the syphon by the rack and pinion, till it delivers 1 gallon in 1 mean solar hour, or 1 pint in $7\frac{1}{2}$ minutes.

"The rate thus adjusted, if the motion of a star is to be followed, place the cord in the upper — if of the moon in the lower — groove of the right-ascension circle, and let it hang by the small plummet over the pulley fixed to one of the legs of the stand.

"The telescope directed to the object, the cord clamped, and the syphon in action, no movement of the instrument will take place till the cord is so stretched as to overcome the friction of the collars in the tube; we, therefore, turn the milled head at the upper edge of the pan, which, by opening the stop-cock, causes the floating weight to sink rapidly; this motion being stopped when the object appears in the centre of the field of view, the action of the syphon alone will cause it apparently to remain stationary.

"I wish it to be distinctly understood that the instrument now submitted to your notice was designed to show (independently of its application to the production of equatoreal motion) the methods by which an extended wire, the axis of a tube, or the axis of a telescope, might be made parallel to the earth's axis to a sufficient degree of exactness to make it available for astronomical observations.

"Other simpler forms might be adopted where one telescope only need be used, or a telescope and its finder, the latter having a field of view of $2^{\circ}\ 57'$ might serve to determine the polar axis, and be so mounted as to produce the equatoreal motion required.

"Where very powerful telescopes are used, the smaller one used

for finding the polar axis might have a field of view of 3°, and the exact position of *Polaris* within the circumference might be determined by a micrometer adapted for the purpose; this would admit of the requisite corrections for refraction.

"Such a telescope attached in the same manner as a finder to the polar axes now used in observatories would serve readily to test the accuracy of their position, while in the Fraunhofer form of equatoreal it may be substituted for the polar axis, when we should obtain the advantage of making the axis of one telescope the fixed axis about which another may be made to revolve.

"As differences in the temperature of the water occasion some little variation of rate in the quantity delivered by the syphon, the rack-work should be graduated for these differences, or self-regulating clepsydras may be constructed either by using two cisterns and two syphons (one of the latter acting as a supply pipe to the cistern carrying the weight connected with the telescope), or if one cistern only be used by a thermometer carrying a piston attached to the syphon, the bulb of the thermometer being fixed under the lower surface of the float.

"If the telescope be not well balanced about its centre of gravity, more or less weight will be required to move it, as its direction is varied. If, however, the float cover a large surface of water, and the syphon, when adjusted for the mean temperature, be at some depth below this surface, no perceptible variation of rate will be occasioned.

"The action of the clepsydra produces a smoothness of motion not to be surpassed; and I feel assured it might be adapted to the requirements of an observatory, more especially as its rate may be so readily brought under the immediate control of the observer."

Description of a Miniature Observatory erected at No. 200 *High Street, Exeter.* (Latitude 50° 43′ 19″·45 N., and longitude $0^h\ 14^m\ 7^s$ W.) By Messrs. Ellis and Son.

(*Communicated by Dr. Lee.*)

This Observatory is built over workshops at the back part of the premises in the above locality, about 40 feet above the ground, and at 175 feet above the mean level of the sea, which elevation commands the northern horizon (48° below pole) as well as 36° of south declination. The upper chamber is 3 feet 1 inch wide (except that portion which falls into the return of the chimney, and which is a little wider), 5 feet 8 inches long, and 6 feet high. It has north and south doors, a chicket window, forming a convenient desk and cupboard, and a skylight, in the direction of the meridian, which is readily opened by means of a short lever. The eastern wall is painted; the others canvassed and papered. The doors and meridional opening are painted black. The roof being flat is covered with zinc.

The transit, a 20-inch instrument by Troughton, is mounted on

a bracket built into the eastern wall, and is as steady as can be expected at such a height in the midst of a crowded city.

A Dent's dipleidoscope, furnished with a telescope, is fixed to the same bracket.

A sidereal clock stands, or rather is hung, in the south-east corner, and an electro-magnetic dial, by Shepherd, occupies the space between it and the south door. The latter shows Greenwich mean time, and is connected with an electro-magnetic pendulum and another dial, in a distant part of the premises, on the ground-floor.

The thermometers, by Negretti and Zambra, have wet and dry bulbs. A patent maximum and a minimum thermometer are arranged on a stand outside the north window.

A Glaisher's rain-guage is erected 10 inches above the roof, and its cistern is also placed outside the north window.

A standard barometer, Barrow's, No. 25, is hung against the eastern wall of the lower platform, and is 164 feet above the level of the sea.

This paper was accompanied by a plan of the Observatory, and a few observations of Comet III., 1853, by Mr. H. S. Ellis.

Notes on the Appearance of the Zodiacal Light in the Spring of 1853. By T. W. Burr, Esq.

The atmosphere being exceedingly transparent, and the horizon clear round my residence at Highbury, I had since January kept watch for the appearance of the zodiacal light, but it was not until Friday, 11th March, that I was able to distinguish it. On that evening the sky was perfectly cloudless and the air calm. The sun and moon had both set by $7^h\ 30^m$ mean time, and the yellowness of the twilight having also disappeared, the desired luminosity became visible in the western part of the heavens. It was a very long cone or lenticular-shaped beam of nebulous milky light, starting from the western part of the horizon, but having more of its base to the north than south of that point. The angle made with the horizon was considerable, being about 45°, and the breadth at the base about 30°. These measures were only approximate estimations. The upper edge of the cone was less distinct than the lower, which was well defined. The point could be traced very nearly to the Pleiades, where it was lost. This group was then about three hours past the meridian, and about 50° in altitude. The point was not rounded off in the manner usually described and drawn, but sharper, so that the whole light looked much like half of the nebula in *Andromeda*, as drawn in Sir J. Herschel's *Outlines of Astronomy*. The stars α, β, and γ *Arietis* were immersed in the beam of light; *Saturn* shone brightly, just beneath its lower edge; and the principal stars of *Andromeda* were beyond the northern boundary. The light appeared generally to follow the line of the ecliptic, and to extend about 65° from the sun's place.

It was equal in brightness to the portion of the Milky Way in *Cepheus* and *Cassiopeia*, then visible; and its presence made a striking contrast between the western sky and the dark blue of the eastern heavens. The light was watched till $8^h\ 30^m$, when it was still visible, but much fainter, and not reaching beyond *Saturn*.

At the close of the Meeting the President stated that operations had commenced for determining the difference of longitude of Brussels and Greenwich by means of galvanic signals, with the view of thereby forming an electric communication between Greenwich and the principal Observatories of the Continent. Observations of signals and of transits had been already made for this purpose at Brussels by Mr. Dunkin of the Greenwich Observatory, and at Greenwich by M. Bouvy, one of the assistants of M. Quetelet, the Director of the Royal Observatory of Brussels. The Observers were subsequently to exchange positions, by which means it was to be hoped that all personal equation, both for the transit observations and for the observation of the signals, would be completely eliminated. With respect to the velocity of the electric current, the President remarked that, in the present instance, there was reason for suspecting it to be affected by the subterranean and submarine passage of a portion of the wire. It appeared that the time occupied by the electric current in passing from Greenwich to Brussels amounted to $\frac{1}{10}$th of a second, whereas the time occupied by the current in passing from Greenwich to Edinburgh, which was at almost double the distance, amounted only to $\frac{1}{17}$th of a second. The velocity on the American telegraphs is still greater. The President referred to experiments instituted by Mr. Latimer Clark, which, in company with Dr. Faraday, he had had the pleasure to witness; which fully supported the idea that the galvanic induction of the surrounding matter, without impairing the insulation (which is sensibly perfect), materially retards the progress of the galvanic current.

The President acknowledged the obliging conduct of the authorities of the European and Submarine Telegraph Company, who had cordially co-operated in promoting the success of this important undertaking.

Note respecting Capt. Toynbee's Paper on the Rating of Chronometers by Lunars.

In reference to the communication of Captain Toynbee, respecting a method for determining the rates of chronometers by lunar observations made at sea, which have appeared in our *Notices*, we may observe (what many of our readers are doubtless aware of) that in the ordinary application of lunar distances taken with a sextant, the longitude corresponding to each set of distances is computed. When, however, chronometers on shipboard

are sufficiently trustworthy, the ship likewise changing her geographical position, in this case it is found to be more expedient simply to register the respective errors of the chronometers from Greenwich mean time, as computed from each set of distances, with the day and hour of observation. When a number of such results are obtained, sufficient to remove as far as possible the unavoidable errors of instruments, as well as personal peculiarities in observing, and these results being at the same time confined within reasonable limits as regards date of observation, then a mean of the errors is found corresponding to the mean date. Thus new errors and rates for the chronometers are determined, as pointed out by Captain Toynbee.

We must observe, however, that this valuable mode of consolidating, as it were, the results of lunar observations when made at sea, and the ship changing her position, and which method Captain Toynbee has so skilfully and successfully applied to practice, is not new. In the well-known Nautical Tables of Mendoza Rios, 2d edition, p. 45 of the explanation to the tables, he will find the method clearly explained, with an example of its application; and M. Rios observes, "The preceding rule and example have been communicated to me by my esteemed friend Captain Huddard, who has successfully made use of that method in his voyages." The same method was extensively applied in the polar expeditions of Capt. Sir E. W. Parry, during the periods of summer navigation; and the longitudes of various places thereby determined. In his second voyage in 1821, 2, 3, it was somewhat modified, by considering the weight of each day's observation to depend upon the number of observations, and also the interval between that day and the date from which the new errors were estimated.

We have now before us a considerable collection of observations, made for the express purpose of determining the lunar rates of chronometers in the South Pacific, and other parts of the world. Also, a printed form, in use at the Naval Schools of Greenwich Hospital, having the same object in view. Discussions of this nature are not, however, without value; if they elicit nothing that is new, yet they bring forward more prominently, and give strong testimony to that which is sound and valuable; and it is gratifying to find officers using the talents and energy with which they are gifted, and *discovering for themselves* the best modes of obtaining results upon which the safety of their ships so much depends.

The *Nautical Almanac* for 1857 has just appeared. This is the first volume of the Ephemeris which has been published since the appointment of the new Superintendent, J. R. Hind, Esq. Besides the usual quantity of matter it contains an extension of the list of Fixed Stars from 100 to 147, and a Supplement containing Ephemerides of all the newly-discovered planets for the year 1854.

Of the forty-seven stars now inserted for the first time, the mean

places of forty-three have been derived from the Greenwich Observations of 1850, as printed, and the observations of 1851 and 1852, as supplied in manuscript by the Astronomer Royal. The positions of γ *Orionis*, h^2 *Sagittarii*, ϱ *Capricorni*, and γ^1 *Virginis*, have been taken from the Greenwich Twelve-year Catalogue, the place for 1840 alone having been adopted for the latter star. The proper motions, as determined by the Rev. R. Main, in his paper on the subject (*Mem. Ast. Soc.* vol. xix.), or computed by similar formulæ, have been included in the reductions of the mean places of the forty-seven additional stars to the year 1857.

Among minor improvements visible in this volume may be mentioned an economising of space, which has been judiciously employed in promoting the usefulness of the Ephemeris.

Determination of the Longitude of Papeété, from Observations of a partial Eclipse of the Sun.

(*Communicated by Rear-Admiral Sir Francis Beaufort.*)

The geographical position of Tahiti was originally determined by Captain Cook on the occasion of the expedition which was despatched under his command by the British Government to observe the transit of *Venus* at that island in the year 1769. The observations of the transit, which occurred on the 4th of June, 1769, were made at a place in the northern extremity of the island, since called Point Venus. The calculations indicated the longitude to be 151° 52′ 54″ relatively to the meridian of Paris.

The observations contained in the present communication were made at the Government House of Papeété (the capital of Tahiti), on the occasion of an eclipse of the sun, which occurred on the 6th of June, 1853. It was intended that observations should also be made at Afaite, one of the neighbouring islands, where the eclipse was to be annular; but the expedition despatched by the Governor for this purpose failed to arrive before the occurrence of the phenomenon.

The eclipse was observed at Papeété by three individuals, viz. by the Governor, by Lieut. Desaux of the French Navy, chief officer of his staff, and by M. Briot, his ordnance officer. Lieut. Boulangé counted the seconds for the three observers upon a marine time-keeper, regulated from observations made by him on the preceding days and two hours before the occurrence of the phenomenon. The sky was very clear.

The precise instant of the separation of the two discs was announced by the three observers as follows:—

	h	m	s
M. Briot	12	51	41·0
M. Desaux	12	51	43·5
The Governor	12	51	44·0

Of the three observations, the two last are entitled to the

greatest confidence, partly on account of their accordance and partly from the superiority of the instruments with which they were made. Their mean, viz. $12^h\ 51^m\ 43^s{\cdot}75$, or in mean time, after all corrections were applied, including the error of the watch, $22^h\ 7^m\ 40^s{\cdot}7$ (June 5), was accordingly employed as the basis of calculation, in which account is taken of the heights of the barometer and thermometer at the time of observation. The following are the observations from which the latter data were deduced :—

	h m	Bar.	Ther. ° ′	
June 5	2 30	0·764,8	26	(Centigrade)
6	2 25	0·764,02	26 3	—

The calculations for the determination of the longitude were executed by M. Adam Kutcyski. They give for the longitude of Papeété : —

	h	m	s
In Time	10	7	34·31
Arc	151	54	4·65

These numbers have reference to the meridian of Paris. They present a satisfactory agreement with the longitude indicated in the *Connaissance des Temps*. In the latter work the longitude of Point Venus is stated to be 151° 49′ 19″. Now M. Kutcyski, by a process of triangulation based upon this determination, found the longitude of the Government House of Papeété to be 151° 54′ 4″·5, which differs only 15 hundredths of a second from the result obtained by means of observations of the eclipse.

On the Parallax of Argelander's Star. By Prof. C. A. F. Peters.*

In this paper M. Peters subjects to a critical discussion the recent researches of M Wichmann on the parallax of Argelander's star (1830 Groombridge), and assigns what he considers to be the most trustworthy results deducible from the observations.

The basis of M. Wichmann's investigation consisted of a series of heliometric measures of the distance of Argelander's star A, from three other stars in its vicinity, a, a', a''.† The star a was on one side of the principal star, A, and the stars a', a'', were on the other, all the four stars being nearly upon the same parallel.

By a discussion of the observed *differences* of the distances of the stars of comparison from the principal star A, M. Wichmann found the parallax of the latter star to be 0″·135, with a probable error of 0″·013, the parallaxes of the stars of comparison being supposed to be insensible.

A similar discussion of the observed *sums* of the distances presented a peculiar difficulty in consequence of the maximum effect

* *Ast. Nach.* No. 865–6.

† The stars are here denoted as in Wichmann's Researches, for an abstract of which see *Monthly Notices*, vol. xiii. p. 131.

produced by the influence of temperature on the instrument, being almost coincident with the maximum effect of parallax. M. Wichmann was, therefore, obliged to adopt two distinct hypotheses in the conduct of this part of his investigation. By the first hypothesis the observed variations of the sums of the distances were supposed to arise entirely from the influence of temperature on the instrument at different seasons of the year; by the second hypothesis, Bessel's coefficient of temperature was introduced into the investigation as a definitive result, and the residual effect was attributed entirely to parallax. The first hypothesis gave for the coefficient of temperature a value greatly exceeding that which Bessel had deduced from his researches on the subject. By the second hypothesis, M. Wichmann found the parallax of a to be $1''{\cdot}17$, and that of A to be $0''{\cdot}71$, assuming the parallaxes of a' and a'' to be insensible.

M. Wichmann is of opinion that the last-mentioned value of the parallax of A ought to be regarded as the most trustworthy which has hitherto been deduced.

Bessel's investigation of the coefficient of temperature was founded on a series of heliometric measures of the distances of *Halcyone* from ten other stars of the *Pleiades*. These measures had been executed by Bessel, Schlüter, and Plantamour, during the interval comprised between the years 1830 and 1840. Denoting the value of this coefficient by ε, Bessel deduced from his researches, $\varepsilon = +0{\cdot}00000378$ with the probable error $0{\cdot}00000045$.

Dr. Peters found, however, that after Bessel's correction for temperature thus found had been applied to these observations, and they were in other respects completely reduced, the outstanding errors *varied with the season of the year*, and (taking into account the distances measured in both cases) were of the same order of magnitude as $1''{\cdot}17$, the value which M. Wichmann obtained for the parallax of the star a by an investigation of the *sums* of the distances of the comparison stars from the principal star A.

These variations could not be explained by the effect of parallax, since, although the stars of comparison lay in every direction round the standard star, the variations for each day of observation always took place in the same direction.

It was evident, therefore, that no reliance could be placed on a result of parallax, the value of which was determined by employing Bessel's correction for the influence of temperature on the instrument.

From Schlüter's measures alone M. Peters determined the value of ε to be $+0{\cdot}00001243$ with a probable error of $0{\cdot}00000073$. Hence it is evident that the uncertainty attending Bessel's determination of the coefficient of temperature is far greater than its probable error indicates. Upon this ground M. Peters maintains that to ascertain beyond doubt the influence of temperature, a series of direct measures of the focal length of the instrument is indispensable. Until these be executed, every determination of parallax which is deduced in such a way that the uncertainty in the value

of ε is capable of exercising a sensible influence on the result, ought to be regarded as illusory.

The value of ε, which M. Peters deduced from Schlüter's observations, considerably exceeds that which M. Wichmann obtained for the same coefficient by a discussion of his own observations of the sum of the distances, upon the hypothesis that the effects of parallax are insensible. M. Peters therefore maintains that no weight can be attached to the objection urged by M. Wichmann to that hypothesis, founded upon the large magnitude which it assigned to the coefficient of temperature, compared with the value of the same element which Bessel deduced from his researches.

M. Peters also found that the values of ε deducible from experimental measures executed by M. Struve with the Dorpat and Pulkowa refractors, were, in both cases, considerably greater than Bessel's value.

Besides a variation depending on the season of the year, and indicating an erroneous value of Bessel's coefficient of temperature, it appeared that the observations, both of Schlüter and Wichmann, were frequently affected with a constant error for each day of observation. This error appeared to be totally independent of the indications of the thermometer, and was found to affect the measures to the extent of 1″ in twenty-four hours.

M. Peters remarks that the outstanding errors by which the sums of the distances are affected when Bessel's coefficient of temperature is employed, and the difference of the parallaxes of the stars of comparison is supposed to be insensible, are attributable partly to an error in the value of the coefficient of temperature, and partly to unknown causes. He is of opinion, however, that the difference between the temperature of the interior of the telescope and the temperature outside, exercises a considerable influence upon these errors.

With respect to the value of the parallax of A, which M. Wichmann deduced from the differences of the observed distances of the stars of comparison, viz., 0″·135, with the probable error 0″·013, M. Peters considers it to be a result entitled to great confidence. In this case the measured distances of the stars of comparison from the principal star being nearly of equal magnitude, any constant error for each day of observation must necessarily almost disappear from their differences, whether arising from the optical imperfection of the instrument, the unequal heating of the air in the interior of the tube, the difference of the temperature within and the temperature outside, the uncertainty in the value of the coefficient of temperature employed in reducing the observations or the combined operation of all these causes.

Although the parallax which M. Wichmann obtained by a discussion of the differences of the distances was admitted by M. Peters to constitute a good approximation to the true value, still it occurred to him that, on account of the importance of the subject, it would be desirable to deduce the same result by such a method that the uncertainty in the value of the thermometric coeffi-

cient, and especially every error calculated to operate proportionally to the measured distance, would be *totally* eliminated. He, therefore, subjected the observations of Schlüter and Wichmann to a process of treatment, similar to that by which on a former occasion he had deduced the parallax of 61 *Cygni*. In this manner he found the relative parallax of A to be $+0''{\cdot}141$, with the probable error $0''{\cdot}013$, and estimating the parallaxes of the stars of comparison by the aid of his former researches on the parallaxes of the stars of different magnitudes, he found the absolute parallax of A to be $0''{\cdot}148$.

This result agrees very nearly with that which M. Wichmann deduced from the differences of the distances, and appears to M. Peters to afford a sufficient confirmation of his remark, that the latter result is influenced in only a very small degree by the uncertainty in the value of the coefficient of temperature, or any other cause of error which may operate proportionally to the distance.

Memorie dell' Osservatorio dell' Universita Gregoriana del Collegio Romano, diretto dai PP. della Compagnia di Gesu. Anno 1851.

This volume contains a record of the labours of Prof. Secchi at the Observatory of the Collegio Romano during the year 1851. It is divided into two Parts. The first relates to astronomical subjects; the second is devoted to observations of a purely meteorological nature. Part I. is divided into four sections. The first section contains the observations with the meridian circle. The second is devoted to observations of comets and of the newly-discovered planets with the Cauchoix refractor. The third contains a variety of physical observations of *Saturn* and his rings. It appears from these observations, that the interesting phenomenon of a difference of colour at the opposite ansæ of the obscure ring, which was noticed by Mr. Lassell during his late residence at Malta,* had been remarked by Prof. Secchi in the autumn of 1851. The following passage, extracted from the record of the observations of the planet, may be cited in relation to this fact:—

"Sept. 2, 1851. The obscure ring is very distinct, the Cassinian division is well defined, and there are some traces of a subdivision of the exterior ring. The obscure ring is more distinct, and somewhat of a rose colour on the left side of the observer; on the right side it appears of a bluish colour."

The fourth section of Part I. contains an account of the researches of Sig. Sestini on the colours of the stars. There is an Appendix to the work, containing an account of researches upon the distribution of heat on the surface of the sun, and also experiments on the azimuthal motion of the pendulum, instituted with the view of proving the rotation of the earth.

* See p. 148 of vol. xiii. of the *Monthly Notices*.

Theorie der Pendelbewegung mit Rücksicht auf die Gestalt und Bewegung der Erde, von P. A. Hansen, Director der Sternwarte in Gotha. (*Neueste Schriften der Naturforschenden Gesellschaft in Danzig, Fünften Bandes Erstes Heft. Danzig,* 1853.)

It is stated on the Title-page that this is a Prize Memoir, which was crowned by the Physical Society of Danzig on the 3d of January, 1853. It contains an investigation of the various circumstances which affect the motion of a pendulum at the surface of the earth, whether we suppose the pendulum to be a mathematical point or a physical agglomeration of particles. In a short introduction, M. Hansen has given an account of some of the more important results to which he was conducted by his researches on the subject.

The Memoir is divided into seven sections. In the first section the author investigates the equations of motion of the simple pendulum, taking into account the figure and motion of the earth. Besides its diurnal rotation, the displacement of the earth, whether arising from its annual motion round the sun, or from the motion of the whole solar system in space, is considered in this part of the inquiry. It is found, however, that in both of the last-mentioned cases, the effect produced upon the motion of the pendulum is insensible.

The investigation of the motion of the pendulum is reduced to that of a freely-moving body by representing the tension of the pendulum-rod as an indeterminate force acting in the direction of its length. Eliminating this force from the differential equations of motion of the pendulum, M. Hansen obtains the equations of motion of a body, whether falling freely or projected from a given point in space. Although there was no novelty in the step, he takes occasion to integrate the equations in these two cases, omitting the effects due to the resistance of the atmosphere and the curvature of the earth's surface. The results are somewhat curious. First, in the case of a body falling freely, it is shown that if the height be very great, the deviation towards the *south* will be considerable. For small heights the displacement of the body in that direction will be imperceptible, but on the other hand it will be found to deviate sensibly towards the *east*. Thus, if a body fall from a height of 100 mètres under the equator, the deviation towards the east will be $0^{m}\cdot02199$, and for the same height in latitude $+45^{\circ}$ the deviation towards the south will be only $0^{m}\cdot000001$, a quantity which is quite insensible.*

M. Hansen next investigates the motion of the body when it is projected in any azimuthal direction whatever, and at any inclina-

* It is said that Oersted was the first who remarked that falling bodies, besides deviating towards the east, deviate also a little towards the south. The fact was confirmed by subsequent experiments, but it remained unexplained until the discussion occasioned by Foucault's experiment led to a knowledge of its true cause, viz. the diurnal rotation of the earth.—Editor.

tion with respect to the horizon; and he assigns the formulæ which the expressions for the co-ordinates assume in the two extreme cases of the initial impulse being parallel and perpendicular to the horizon. When the impulse is wholly horizontal, he shows that to an observer considering the body from the origin of motion, it will deviate always from the *left* to the *right* of the plane of projection, no matter what be the direction of the latter in azimuth. When the direction of the impulse is wholly vertical, the body after an assignable time will return to a position in the horizontal plane, from which it was projected, situate a little towards the *west* of its original position. In this case, then, the effect is the reverse of that produced when the body is allowed to fall freely; it is also considerably greater. Thus, if the body attains a maximum altitude of 100 mètres, the deviation towards the west under the equator will be $0^m{\cdot}264$, whereas in the case of a body falling freely from the same height, the easterly displacement was found to be only $0^m{\cdot}022$, or barely one-eleventh of the former quantity.

M. Hansen commences the second section by a process of integration which furnishes him with a complete differential equation of the first order, relative to the motion of the simple pendulum, and he then proceeds to investigate the differential equations of motion for the compound or physical pendulum. In this case the rotatory motion of the pendulous mass is referred to a system of rectangular co-ordinates having the origin at the point of suspension, one of these axes being conceived to pass through the centre of gravity of the pendulum. With a view to simplify the problem, M. Hansen supposes the axes of rotation to be *principal axes*, and he further assumes that the moments of inertia about the axes which are perpendicular to the line passing through the centre of gravity and the point of suspension, or third axis of rotation, are equal to each other. This limitation obviously includes among various forms of construction, the pendulum as usually employed in experiments relative to the rotation of the earth, which has generally consisted of a homogeneous spherical ball, suspended from a fixed point by a slender thread or wire.

By integrating the equations of motion, M. Hansen obtains two *complete* differential equations of the first order, and a third equation of a similar kind, the terms of which are *imperfectly* divested of the integral sign. From one of the former of these equations it appears that, independently of any initial rotatory movement, the pendulum in the course of its oscillations will acquire a motion of rotation about the axis passing through the centre of gravity and the point of suspension. It is shown further that this rotatory motion is not of an oscillatory nature, but has a tendency to go on continually in the same direction.

In the third section, M. Hansen obtains a first approximation to the complete integrals of the equations of motion of the pendulum. First, considering the times during which the successive oscillations are performed, he shows that they are modified not only by the diurnal rotation of the earth, but also *by the rotation*

of the pendulum about its own axis. The correction, however, which is due to each of these causes, is so small in all actual experiments with the pendulum as to be quite insensible. Supposing the pendulum to consist of a very small homogeneous spherical body, suspended by a thread or slender metallic wire, and to perform one of its oscillations in a second of time, the correction depending on the rotation of the earth would be less than the three thousand millionth part of a second. Even if the pendulum extended to the enormous length of 900 mètres, the correction to the time of oscillation—which would in this case amount to 30 seconds —would be less than the hundred thousandth part of a second. Similar results are obtained with respect to the correction depending on the rotation of the pendulum about its own axis, supposing the diameter of the pendulum relative to its length to be small, and the velocity of rotation to be inconsiderable. Thus, supposing the diameter of the pendulum to be one-fortieth of its length, as in the case of the original experiment of Foucault, M. Hansen found that for a correction amounting to the twenty thousandth part of a single oscillation, the pendulum would require to make forty complete revolutions upon its axis during the time of each oscillation; a velocity of rotation so considerable that there would be no difficulty in guarding against it.

M. Hansen proceeds next to consider the path described by the pendulum in space. Confining his investigation to terms of the second order, he finds that the plane of oscillation is subject to a continual variation in azimuth depending on the diurnal rotation of the earth, the direction of azimuthal motion being always from east to west through south, or contrary to the diurnal motion of the earth, and the velocity of revolution being equal to the angular velocity of the earth around its axis multiplied by the latitude of the place of observation.

It appears further, from the researches of M. Hansen, that a *rotatory movement of the pendulum about its own axis causes the plane of oscillation to vary continually in azimuth.* The effect produced in this case will tend to diminish or to increase the azimuthal variation depending on the diurnal rotation of the earth, according as the pendulum rotates upon its axis in the direction of the earth's diurnal rotation, or in the opposite direction.

The pendulum is constrained, by the circumstances above mentioned, to pursue a path which does not anywhere intersect the vertical line, passing through the point of suspension. M. Hansen describes its movement by stating it to revolve upon the surface of an elliptical cone, the axes of the base of which continually vary in position with respect to the meridian of the observer, and he assigns the conditions which determine the magnitude and position of the axes of the ellipse.

M. Hansen found, that by no means a very considerable rotation of the pendulum about its axis would be necessary to produce an azimuthal variation of the plane of oscillation equal to or even exceeding the effect depending on the diurnal rotation of the earth.

Hence it is manifest, that from this cause the plane of oscillation might revolve in a direction actually opposite to that in which it would otherwise revolve. The amount of azimuthal motion depends in this case, not on the absolute length of the pendulum, but on the proportion of the diameter to the length; and, *cæteris paribus*, it gradually becomes less as the diameter of the pendulum is diminished. M. Hansen accordingly recommends, that in all experiments of this kind, the pendulum should be constructed of some substance having a high specific gravity, such as lead; and he suggests that care should be especially taken to guard against a rotatory motion of the pendulum upon its axis.

M. Hansen next considers the case in which the pendulum revolves upon the surface of a cone with a circular basis. Supposing the rotation upon its axis to vanish, he finds that the time which the pendulum will take to accomplish a complete revolution upon the surface of the cone will be different, according as the pendulum revolves from east to west through south, or in the opposite direction. For any place of observation in the northern hemisphere the time of revolution will be less in the former case than it will be in the latter; for any place in the southern hemisphere the result will be the reverse of this.

The difference between the times of a complete revolution of the pendulum, according as it revolves in one or the other of the directions just mentioned, is found to be independent of the initial elongation of the pendulum from the vertical direction; it is also found to change very slowly when the basis of the cone differs only in a slight degree from a circle. Supposing the length of the pendulum to be 10 mètres and the latitude to be 51°, M. Hansen found that, in the case of a pendulum revolving upon the surface of a cone, the base of which is perfectly circular, the difference between the times of a complete revolution, according as it moves in one direction or in the other, amounts to $0^s{\cdot}00073$. This is, indeed, a very small quantity. It may, however, be rendered perceptible by the accumulated effect of many revolutions, as the experiments of M. Bravais have shown.

M. Hansen finally deduces the following theorem from his first approximation to the integrals of the equations of motion of the pendulum:—"*If, at the beginning of the motion, a plane having a fixed position with respect to the mass of the pendulum be conceived to pass through the axis of the latter, and also through the vertical line [in which the point of suspension is situate], this plane will continue to pass through the vertical line at each successive maximum elongation of the pendulum, notwithstanding the azimuthal motion of the plane of oscillation; provided that, at the beginning of motion, no impulse communicating a rotatory motion about its axis be applied to the pendulous body.*" The plane referred to will, in fact, always pass through the vertical line, abstracting from consideration a small periodical deviation. "Thus," says the author, "in like manner, as the moon—although from a different cause—always turns the same side towards the earth, with

the exception of the slight fluctuations due to the effect of libration, so does the pendulum exhibit the same surface to an eye situate in the vertical, and viewing it in any direction whatever."*

In the fourth section M. Hansen investigates the effects deducible from further approximations to the integrals of the equations of motion. In this part of his researches he employs the theory of the variation of arbitrary constants.† From the second approximation he derives a term expressing an azimuthal motion of the plane of oscillation, and depending, for any given length of the pendulum, on the product of the maximum and minimum elongations from the vertical. If no lateral impulse be communicated to the pendulum at the commencement of the oscillations, the azimuthal motion of the plane of oscillation (or, which amounts to the same thing, the azimuthal motion of the axes of the ellipse), as already determined by the first approximation, will not be sensibly affected by the additional term. When this condition, however, is not satisfied, the result will be different, but it may be found by the aid of the following consideration. Since any lateral impulse directly affects the semi-minor axis of the ellipse, we are enabled, from observations of the magnitude of the latter in any experiment, to infer, *à posteriori*, the intensity of the lateral force, and also the quantity of the azimuthal motion of the plane of oscillation depending on its influence. Supposing the length of the pendulum to be 10^m, and the greatest and least elongations from the vertical to be $\frac{1}{9}$ and $\frac{1}{400}$‡ respectively, M. Hansen finds that the additional quantity of azimuthal motion resulting from the second approximation would then amount, for any place of observation in Central Europe, to one-fourth of the effect depending on the rotation of the earth, as already determined by the first approximation. It is manifest, therefore, that a much smaller value of the minimum elongation than that above-mentioned would suffice to exercise a sensible influence on the motion of the plane of oscillation.§

The term above referred to is the only one among those fur-

* From the terms in which M. Hansen alludes to this curious principle (*ich bekomme ferner einen neuen Satz*), he appears to think that it had been hitherto unknown. It is right to mention, however, that soon after the announcement of Foucault's experiment, the theorem in question was pointed out to be a necessary consequence of the rotation of the earth; and it was also said to have been fully verified by subsequent experiments with the pendulum. (See for example, *Phil. Mag.*, June 1851, also *Mec. Mag.*, vol. liv., p. 326.) It is not improbable, however, that M. Hansen may have been the first who demonstrated the theorem by an analytical process.

† In the *Philosophical Magazine* for October 1851, there is a paper by the Rev. J. A. Combe, "*On the Motion of the Apse Line in the Pendulum Oval*," containing an elegant application of the theory of the variation of arbitrary constants. The rotation of the earth, however, is not taken into consideration.

‡ These fractions are supposed to refer, not to the mètre, but to the length of the pendulum as the standard unit.

§ For an investigation of the effect produced on the motion of the axes of the ellipse (without reference to the rotation of the earth), when the oscillations are nearly circular, and when they are almost in the same plane, see two papers by Mr. Airy, in vols. xi. and xx. of the *Mem. R. Ast. Soc.*

nished by the second approximation which M. Hansen found to be capable of producing a sensible effect.

In the fifth section M. Hansen considers the effect of the resistance of the atmosphere on the motion of the pendulum. The investigation is conducted upon two distinct hypotheses respecting the law of resistance, namely, when it is proportional to the simple power of the velocity, and when it is proportional to the square of the velocity. The effect is also considered when the oscillations are nearly in the same plane, and when they are nearly circular. The results deduced from the two hypotheses are essentially different, insomuch that observations instituted for the purpose, might not improbably indicate the hypothesis which most nearly represents the actual effect.

The sixth section of M. Hansen's Memoir is devoted to an investigation of the effect of the torsion of the thread or wire by which the pendulum is suspended.

In the seventh section M. Hansen proposes a new form of the pendulum, designed to obviate certain disadvantages attending the usual mode of construction. He remarks, that if the pendulum be suspended in the ordinary way, as described by him in the preceding part of his Memoir, it is indispensable, in order to avoid other sources of disturbance than those which he has investigated, that the thread or wire by which the pendulum is suspended should be homogeneous in its structure, and that its transverse section should be everywhere exactly circular. Especial care should be taken that the thread, in consequence of the pressure at its upper end, should not lose its circular-cylindrical form, since, from this cause alone, the variation of the plane of oscillation would be different in different azimuths. The stiffness of the thread also exercises an influence upon the law of the diminution of the arcs of oscillation which would otherwise result from the resistance of the atmosphere. M. Hansen, accordingly, is of opinion, that the following construction of the pendulum, inasmuch as it is free from those disturbing influences, is preferable to the usual form.

Instead of a slender wire, let the pendulum be suspended by a metal rod, so thick as to preclude all possibility of torsion. Let the ball be attached to the lower end of the rod, so that, if the axis of the latter were prolonged, it would pass through the centre of gravity of the ball. Let the rod be connected at the upper end with an apparatus of metal, terminating underneath in a spherical segment of small radius, the centre of which also lies in the prolongation of the axis of the pendulum rod. This spherical segment, composed of hardened steel or hard stone, being placed upon a horizontal plane surface of the same material, will constitute the point of suspension, as in earlier apparatuses the pendulum was made to swing by being suspended upon a knife-edge resting on a horizontal plane. It is intended that, during the oscillations of the pendulum, the supporting spherical segment shall *roll*, but not *slide*. For small arcs — of which only there is here any question — this object will be attained by merely smoothing the surfaces of the

spherical segment and its supporting plane without communicating to them a high polish.

M. Hansen remarks, that the form of construction here proposed by him especially possesses the advantage that, when the oscillations lie in one plane, or nearly so, a rotation of the pendulum about its axis (which he had shown to arise readily from the azimuthal motion of the plane of oscillation depending on the diurnal rotation of the earth) is no longer possible. The remainder of the section is occupied with an investigation of the motion of such a pendulum.

On the Secular Variation of the Moon's Mean Motion.

By J. C. Adams, Esq., M.A. F.R.S. &c.*

The author remarks, that in treating a great problem of approximation, such as that presented to us by the investigation of the moon's motion, experience shows that nothing is more easy than to neglect, on account of their apparent insignificance, considerations which ultimately prove to be of the greatest importance. One instance of this occurs with reference to the secular acceleration of the moon's mean motion. Although this acceleration and the diminution of the eccentricity of the earth's orbit, on which it depends, had been made known by observation as separate facts, yet many of the first geometers altogether failed to trace any connexion between them, and it was not until he had made repeated attempts to explain the phenomenon by other means, that Laplace himself succeeded in referring it to its true cause.

The accurate determination of the amount of the acceleration is a matter of very great importance. The effect on the moon's place, of an error in any of the periodic inequalities, is always confined within certain limits, and takes place alternately in opposite directions within very moderate intervals of time, whereas the effect of an error in the acceleration goes on increasing for an almost indefinite period, so as to render it impossible to connect observations made at very distant times.

In the *Mécanique Céleste*, the approximation to the value of the acceleration is confined to the principal term, but in the theories of Damoiseau and Plana, the developements are carried to an immense extent, particularly in the latter, where the multiplier of the change in the square of the eccentricity of the earth's orbit, which occurs in the expression of the secular acceleration, is given to terms of the seventh order.

As these theories agree in principle, and only differ slightly in the numerical value which they assign to the acceleration, and as they passed under the examination of Laplace, with especial reference to this subject, it might be supposed that only some small numerical rectifications would be required in order to obtain a very exact determination of this value.

It has not been, therefore, without surprise, which he has no doubt will be shared by the Society, that the author has lately

* Proc. of the Roy. Soc. June 16, 1853.

found that Laplace's explanation of the phenomenon in question is essentially incomplete, and that the numerical results of Damoiseau's and Plana's theories, with reference to it, consequently require to be very sensibly altered.

Laplace's explanation may be briefly stated as follows. He shows that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but also on the eccentricity of the earth's orbit. Now this eccentricity is at present (and for many ages has been) diminishing, while the mean distance remains unaltered. In consequence of this, the mean disturbing force is also diminishing, and therefore the moon's gravity towards the earth at a given distance is, on the whole, increasing. Also the area described in a given time by the moon about the earth is not affected by this alteration of the central force; whence it readily follows that the moon's mean distance from the earth will be diminished in the same ratio as the force at a given distance is increased, and the mean angular motion will be increased in double the same ratio.

This, the author states, is the main principle of Laplace's analytical method, in which he is followed by Damoiseau and Plana; but it will be observed that this reasoning supposes that the area described by the moon in a given time is not permanently altered, or, in other words, that the tangential disturbing force produces no permanent effect. On examination, however, he remarks it will be found that this is not strictly true, and he proceeds briefly to point out the manner in which the inequalities of the moon's motion are modified by a gradual change of the disturbing force, so as to give rise to such an alteration of the areal velocity.

As an example, he takes the case of the *variation*, the most direct effect of the disturbing force. In the ordinary theory, the orbit of the moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the sun, and the areal velocity generated while the moon was moving from quadrature to syzygy, would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused, depends in some degree on the eccentricity of the earth's orbit; and as this is continually diminishing, the disturbing forces at equal intervals before and after conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction, and therefore the effects of the tangential force before and after conjunction no longer exactly balance each other.

The other inequalities of the moon's motion will be similarly modified, especially those which depend, more directly, on the eccentricity of the earth's orbit, so that each of them will give rise to an uncompensated change of the areal velocity, and all of these must be combined in order to ascertain the total effect.

Since the distortion of the orbit just pointed out is due to the

change of the disturbing force consequent upon a change in the eccentricity of the earth's orbit, and the action of the tangential force permanently to change the rate of description of areas, is only brought into play by means of this distortion, it follows that the alteration of the areal velocity will be of the order of the square of the disturbing force multiplied by the rate of change of the square of the eccentricity. It is evident that this alteration of areal velocity will have a direct effect in changing the acceleration of the moon's mean motion.

Having thus briefly indicated the way in which the effect now treated of originates, the author proceeds with the analytical investigation of its amount. In the present communication, however, he proposes to confine his attention to the principal term of the change thus produced in the acceleration of the moon's mean motion, deferring to another, though he hopes not a distant opportunity, the fuller treatment of this subject, as well as the determination of the secular variations of the other elements of the moon's motion, which, arising from the same cause, have also been hitherto overlooked.

In the usual theory, the reciprocal of the moon's radius vector is expressed by means of a series of *cosines* of angles formed by combinations of multiples of the mean angular distance of the moon from the sun, of the mean anomalies of the moon and sun, and of the moon's mean distance from the node; and the moon's longitude is expressed by means of a series of *sines* of the same angles, the coefficients of the periodic terms being functions of the ratio of the sun's mean motion to that of the moon, of the eccentricities of the two orbits and of their mutual inclination.

Now, if the eccentricity of the earth's orbit be supposed to remain constant, this is the true form of the expressions for the moon's co-ordinates; but if that eccentricity be variable, the author shows that the differential equations cannot be satisfied without adding to the expression for the reciprocal of the radius vector, a series of small supplementary terms depending on the *sines* of the angles whose *cosines* are already involved in it, and to the expression for the longitude, a series of similar terms depending on the *cosines* of the same angles; all the coefficients of these new terms containing as a factor the differential coefficient of the eccentricity of the earth's orbit taken with respect to the time.

The author first determines as many of these terms as are necessary in the order of approximation to which he restricts himself, and then takes them into account in the investigation of the secular acceleration. The expression which he thus obtains for the first two terms of this acceleration, is,

$$-\left(\frac{3}{2}m^2-\frac{3771}{64}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt.$$

According to Plana, the corresponding expression is

$$-\left(\frac{3}{2}m^2-\frac{2187}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt.$$

It will be observed that the coefficient of the second term has been

completely altered in consequence of the introduction of the new terms.

The numerical effect of this alteration is to diminish by 1″·66 the coefficient of the square of the time in the expression for the secular acceleration; the time being, as usual, expressed in centuries.

It will, of course, be necessary to carry the approximation much further, in order to obtain such a value of this coefficient as may be employed with confidence in the calculation of ancient eclipses.

Considerations of a General Nature relative to the small Planets situate between Mars and Jupiter. By M. Le Verrier.*

M. Le Verrier commences this brief communication by remarking that the hypothesis of Olbers respecting the origin of the small planets, which the great inclination of the orbit of *Pallas* did not serve to confirm, has been completely negatived by the numerous discoveries of recent years. It is to be hoped, he adds, that, since the hypothesis of Olbers must be renounced, the knowledge of a great number of such bodies may finally lead to the discovery of some law regulating their distribution in space. He then proceeds to some general considerations respecting the twenty-six planets already discovered, omitting all reference to the twenty-seventh planet which had been recently discovered by Mr. Hind.

The small planets revolve in a zone which commences at the mean distance of 2·20 from the sun, and extends to the distance 3·16. The mean value of their excentricities is 0·155. The magnitude of the excentricity in individual cases does not seem to have any relation either to the mean distance from the sun or to the position of the perihelion.

The inclinations of the orbits, whether with respect to each other or to the ecliptic, are in both cases considerable. The individual magnitude of the inclinations does not appear to depend either on the mean distance from the sun or on the direction of the ascending node.

The perihelia and the ascending nodes offer some peculiar features. *Twenty* of the perihelia have their longitudes included between 4° and 184°. *Twenty-two* of the ascending nodes have their longitudes between 36° and 216°. M. Le Verrier appears somewhat inclined to suspect the existence of a systematic difference between the mean direction of the ascending nodes of the planets nearer the sun and that of the ascending node of the planets that are more remote, indicating the existence of two distinct groups. He then proceeds to investigate the superior limit of the total quantity of matter which may be distributed in the zone of the heavens under consideration.

An inquiry of this nature can only be conducted by an exa-

* *Comptes Rendus*, Nov. 28, 1853.

mination of the action of such a mass of matter on the neighbouring planets *Mars* and the Earth. It is manifest that the periodic inequalities resulting from the action of the small planets (whose aggregate mass it is proposed to determine) cannot fail mainly to destroy each other by their opposite effects at any assigned instant, and, therefore, must form an unsuitable groundwork for such an investigation.

The secular inequalities being independent of the relative positions of the planets, are not attended with the inconvenience to which reference has just been made, M. Le Verrier remarks that the secular variation of the perihelion of the Earth or *Mars* contains two terms depending on the action of the small planets, one of which is especially adapted for determining the aggregate of their mass. This term depends exclusively on the mean distance of the disturbing planet from the sun, and on the excentricity of the disturbed planet; further, it is essentially positive. It is manifest, therefore, that if the zone, which includes the small planets, contains an innumerable number of such bodies, their respective masses will act together upon the perihelion of the disturbed planet in the same manner as if they were concentrated into one single mass acting at a suitable distance.

With respect to the second term, which is of the same order of magnitude with the first, M. Le Verrier remarks that it depends on the excentricity and the longitude of the perihelion of the disturbing body. In the case of the Earth, since its perihelion is placed almost in the middle of the region of the heavens occupied by the perihelia of more than three-fourths of the small planets, this term might become sensible and contrary in sign to the first term, more especially as the mean excentricity of the small planets is nine times as great as the excentricity of the terrestrial orbit. M. Le Verrier, upon this ground, rejects the secular variation of the terrestrial perihelion as incapable of furnishing a definitive solution of the question under consideration, although its actual value has been ascertained with greater accuracy than that of *Mars*.

With respect to the perihelion of *Mars*, it is more favourably situated relatively to the perihelia of the small planets; moreover, the excentricity of this planet is considerable. By reason of both these circumstances, it happens in this case that the second term does not amount to more than one-fourth of the first term, and it may be expected that this superiority of the first term will continue to subsist after numerous future discoveries of small planets.

Agreeably to these remarks, M. Le Verrier finds that if the aggregate of the masses of the small planets equalled the Earth's mass, it would occasion a variation of the heliocentric longitude of the perihelion of *Mars* amounting to eleven seconds of space in a century. Now, if a variation amounting to even the fourth of this quantity affected the perihelion of the planet, it could not fail to be detected by astronomers, and as no evidence of such a variation exists, it follows as a necessary consequence, *that the total quantity of matter composing the small planets situate between the*

mean distances 2·20 and 3·16 cannot amount to more than about a quarter of the Earth's mass.

M. Le Verrier remarks that a similar result might be obtained by a consideration of the secular variation of the plane of the ecliptic, but in this case the limit would not be so confined.

CONTENTS.

	Page
Fellows elected	33
Discovery of a new Comet by M. Klinkerfues	33
Elements of ditto	33
Observations of Comet III. 1853, by Commander Bradshaw	33
Second Elements of *Euterpe*, by Prof. Chevallier and M. G. Rümker ...	34
Observations of *Euterpe*, by Mr. Carrington	35
Remarks respecting a Phenomenon observed during the apparition of Comet III. 1853, by M. C. Rümker	35
Extracts of a Letter from Prof. C. Piazzi Smyth	38
Remarks by Mr. Carrington on presenting to the Society nine Circumpolar Charts	40
On a new Mode of Mounting a Telescope equatoreally, by Mr. Joseph Hopkins	41
Description of a Miniature Observatory, by Messrs. Ellis and Son, (communicated by Dr. Lee)	44
Remarks on the Zodiacal Light, by Mr. Burr	45
Statement made by the President respecting the Operations for Determining the Difference of Longitude of Greenwich and Brussels by Galvanic Signals	46
Note on the Rating of Chronometers by Lunars	46
Nautical Almanac for 1857	47
On the Determination of the Longitude of Papeété by means of an Eclipse of the Sun	48
On the Parallax of Argelander's Star, by M. Peters	49
Observations made at the Observatory of the Collegio Romano in the year 1851, by Prof. Secchi	52
On the Theory of the Motion of the Pendulum, taking into account the Rotation of the Earth, by M. Hansen	53
On the Secular Variation of the Moon's Mean Motion, by Mr. Adams ...	59
Considerations of a General Nature relative to the small Planets situate between *Mars* and *Jupiter*, by M. Le Verrier	62

London:—Printed by G. Barclay, Castle St. Leicester Sq.